

May 25, 1962

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Dear Helen:

I was pleased to find your corrected paper waiting for me upon our return from Japan. Your suggestions are all excellent ones and I think the paper should just go in substantially as it stands. I do not think it is too long at all by the usual standards of the journal. Perhaps the main comment I should make for accurate information concerns the references. EM agar is mentioned by Hirota in footnote 11 and also by Richter, 1961, page 335. But it is probably better to just replace the reference by "EMS agar without succinate". As for Ann Cook's paper, it should be 1962, and I hope will be published in Genetics. This can be perfected in the proof. I will put in the proper designations for the three lac<sup>-</sup> cultures in table 1. I hope you will not object to my changing the symbol tyro<sup>-</sup> to tyr<sup>-</sup>, only because it is more compatible with our current usage. There are perhaps just one or two other minor changes.

On the whole I am very much pleased at the present shape of the paper and its further improvement by your own comments. I will look forward to seeing the manuscript of the second paper before too long. However, there would be little point in trying to rush it before about the third week in June, as I have a very busy time ahead in catching up with accumulated work.

It is also very gratifying to see how you have continued your studies, although as we had already begun to suspect there may be some serious technical problems in maintaining useful motility and high development of flagella at the same time.

In Japan we had an especially time with Tetsuo Iino, who is continuing his work along more and more physiological and biochemical lines. I hope it will be feasible to arrange to remain in close touch among all of us. In particular we would like to start some DNA studies in connection with phase variation during a brief visit that he may pay to the States in early September. One of the very useful things if it could be possible to develop it before that time would be a *Salmonella serratia* hybrid in which a *serratia* F<sup>-</sup> has acquired and can successfully manifest a small segment from *Salmonella* including H1 and H2. If such a heterogenote can possibly be obtained and if it shows phase variation

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we would like to make a beginning attempt to discover whether there is any difference in the DNA composition of the bacteria in the alternative phases. May I ask whether you would be able to offer anything in this regard? We have been stressing this kind of work very heavily lately particularly with the *Bacillus subtilis* transformation system. Gan has had some very exciting results on the separation of the DNA corresponding to different genes in this system. At the present moment he is in India revisiting his parents, but he will be back in another month or so.

To return to our paper, it will probably expedite matters if I arrange to take care of proof here. Of course, I will see to it that you are informed about the progress of the manuscript and consulted if any material questions should arise. I will also arrange for a reasonable number of reprints to be sent directly to you.

With best regards to Ollie,

Yours sincerely,

Joshua Lederberg  
Professor of Genetics

JL/jc